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MINERALOGY.—*Aurichalcite from Big Cottonwood Canyon, Salt Lake County, Utah.* A. LEDOUX, University of Brussels. (Communicated by T. L. Walker.)

The mineral aurichalcite was first given rank as a distinct species in 1839 by Böttiger. Prior to that time it was recognized as early as 1788 under the name "calamine verdâtre" by Patrin, who mentioned that this variety of calamine contained "une bonne quantité de cuivre."

On account of the character of the mineral, previous investigators have worked at a decided disadvantage in determining the crystal system to which this mineral belongs, and the writer has labored under some of the disadvantages that in the past prevented the most accurate determination of the crystal constants.

A brief résumé of the crystallographic work that has been done on the mineral follows:

In 1874 Des Cloizeaux[1] described aurichalcite as acicular crystals of undetermined form.

In 1890 Albin Belar[2] considered the mineral as monoclinic with $\beta = 0$ and $a : b : c = 1 : x : 0.7208$. He states that the plates of the mineral are parallel to the principal cleavage taken as 010, the side GH of the triangle (fig. 1) being the combination of 010 and 100, the hypotenuse GI being the edge between

[1] Manuel de Minéralogie, **2**: 183. 1874.

[2] *Ueber Aurichalcite.* Zeitschr. für Krystallographie, No. 17. 1890.

010 and a dome. Belar observed the following forms: (104), (101), (201), (301), (401), (501), (601).

In 1896 Prof. G. Cesàro[3] described fragmentary crystals found at Flémalle in Belgium. He considers the mineral as orthorhombic on account of its optical properties. The principal tabular cleavage was by him taken as the front pinacoid (100). The frequency of wedge-shaped tables is due to a pyramidal hemihedrism. In order to obtain simple symbols for the domes Professor Cesàro started from a unit brachydome much steeper than that of Belar and corresponding to

$$c = 8.71263.$$

In 1897 Prof. G. B. d'Achiardi[4] ascribed the wedgelike crystals of aurichalcite to twinning of simpler monoclinic individuals, the lateral faces of the wedges being contact faces of the twins. The following constants were determined:

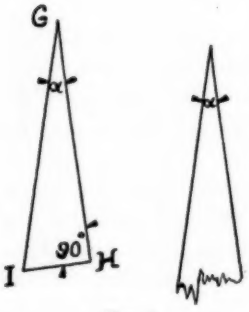

FIG. 1

$$a : b : c = x : 1 : 1.6574$$
$$\beta = 84° 15'.$$

In 1908 M. L. F. Navarro,[5] of Madrid, measured with the microscope five crystals of aurichalcite from Ondárroa, Vizcaya, Spain. The face-angles of the three-pinacoid measured on three crystals gave the following measurements:

$$(a) = 82° 34' \pm 11'$$
$$(b) = 80° 35' \pm 21'$$
$$(c) = 77° 55' \pm 16'$$

angles which may be regarded as α, β, and γ or their supple-

[3] *Description des Minéraux phosphatés, sulfatés, et carbonatés du Sol Belge.* Mém. de l'Acad. Roy. des Sciences, etc., de Belgique, Bruxelles, **53**: 1–134. 1897.

[4] *Aurichalcite de Campiglia Maritima et Valdaspar.* Atti Soc. Tosc. di Sci. Nat. Memori, **16**: 1–15. 1897–98.

[5] Bol. R. Soc. Esp. Hist. Nat. Madrid, pp. 117–119. February, 1908.

ments. Aurichalcite has thus been considered orthorhombic, monoclinic, and triclinic.

Professor A. Lacroix[6] agrees with Prof. Cesàro in regarding aurichalcite as orthorhombic. He suggests that Professor d' Achiardi in regarding the mineral as monoclinic on account of an extinction of 17° may have made his observations on plates seen obliquely to the cleavage. The aurichalcite from Chessy, France, occurs as needles without flattening, with parallel extinction, suggesting orthorhombic symmetry.

Chemical examination of aurichalcite from Utah has been made by Penfield,[7] who deduced the formula $2(Zn, Cu)CO_3 . 3(Zn, Cu)(OH)_2$. If Zn : Cu = 5 : 2, the theoretical composition is as following:

CO_2	16.14
CuO	20.79
ZnO	53.17
H_2O	9.90
	100.00

Penfield did not give any information about the geometrical properties of the material analysed.

The mineral aurichalcite is pale blue to pale green in color, appearing macroscopically to consist of delicate aggregates of bluish needles; under the microscope, however, the needles commonly appear as wedges with a very acute angle α. Sometimes the wedge has the form of a right-angled triangle, but generally the shortest side, opposite the angle α, is replaced by an irregular line (fig. 1).

On account of the uncertainty of the symmetry of aurichalcite, I thought it of some interest to make further investigation of its crystals. Some very fine specimens from Big Cottonwood Canyon, Salt Lake County, Utah, were placed at my disposal by the Royal Ontario Museum of Mineralogy. The structure is usually radial or plumose. To study the mineral it is first of all necessary to get simple individuals; these are most easily obtained by crushing an aggregate between two glass

[6] Minéralogie de la France et de ses colonies, **3**: 739. 1909.
[7] Amer. Journ. Sci., III, **41**: 106. 1891.

slides. One obtains then a great number of small individuals of various forms, the most common of which is triangular in outline (fig. 1). The angle α is very acute, while the angle GHI is 90°. In several individuals from Big Cottonwood Canyon the value of α is 19°, a value agreeing with that obtained by Professor Cesàro[8] on the crystals of Flémalle. Other forms observed on these specimens are shown in figure 2. Most of the

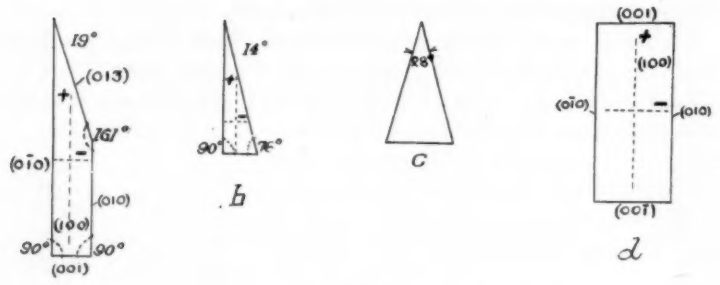

FIG. 2

individuals are cleavage plates with a contour of pinacoids and domes belonging to the zone (001) (010). If (0kl) is the notation of one of those domes, the angle $\alpha = (0kl \wedge 010)$ may be deduced from the equation:

$$\frac{l}{k} = c. \tan \alpha$$

As the angle $\alpha = (013 \wedge 010)$ has the same value of 19° as that given by Professor Cesàro we may use the same parameter,

$$c = 8.71263$$
$$\log c = 0.94015.$$

Table 1 gives the different α angles observed on the crystals and the corresponding symbols of the domes; but the results are only approximate, as the angles were measured with the microscope.

[8] Loc. cit.

TABLE 1

ANGLES OBSERVED FOR AURICHALCITE AND THE CORRESPONDING SYMBOLS OF THE DOMES

α	$\log \frac{l}{k}$	$\frac{l}{k}$	O k l
6°	1.96177	0.9157	0 1 1
10°	0.18647	1.356	0 2 3
14°	0.83692	2.173	0 1 2
19°	0.45279	2.837	0 1 3
23°	0.53203	3.404	0 2 7
27°	0.59720	3.955	0 1 4
30°	0.70159	5.030	0 1 5
44°30′	0.93257	8.562	0 2 17

There are probably also rhombic pyramids elongated in the direction of the vertical axis with very short intercepts on the a axis. It is generally impossible to focus exactly these faces, and as a result the a parameter cannot be calculated, nor the symbols of the pyramidal faces. It sometimes happens that an individual of aurichalcite is lying on one of these pyramidal faces; the extinction angles and the observations in convergent light become then valueless for the determination of the crystal system.

As to the optical properties of the aurichalcite it may generally be noticed that the extinction is straight along the (010) (100) edge. The acute bisectrix is normal to the cleavage plate (100), which exhibits mean birefringence. The crystal plates are so small that it was impossible to measure the indices of refraction and the other optical properties.

MINERALOGY.—*The indices of refraction of analyzed rhodochrosite and siderite.*[1] EDGAR T. WHERRY, National Museum, and ESPER S. LARSEN, Geological Survey.

Rhodochrosite. The beautiful, transparent, crystallized rhodochrosite from the John Reed Mine, Alicante, Lake County, Colo., has been described by Dr. G. F. Kunz[2] who gave the FeO

[1] Published with permission of the Secretary of the Smithsonian Institution and the Director of the U. S. Geological Survey.

[2] Am. Journ. Sci., (3) **34**: 477. 1887.

content as 3.62 per cent, and the specific gravity 3.69, on the authority of J. B. Mackintosh. Prof. E. S. Dana states[3] that the angle $r \wedge r'$ is 73° 4½′. In the course of the determination of the optical constants of minerals one of us (E. S. L.) desired to obtain the indices of refraction of the purest possible rhodochrosite and in order to confirm the composition of the mineral in this occurrence, a specimen (No. 86249) in the National Museum collection was analyzed (by E. T. Wherry). The results of the analysis were: $MnCO_3$ 95.72, $FeCO_3$ 1.87, $CaCO_3$ 0.50, $MgCO_3$ 0.68, gangue 0.82 per cent, sum 99.59 per cent. This represents a lower percentage of iron than previously reported, but undoubtedly the relative amounts of isomorphous constituents vary from one specimen to another. That this specimen is actually somewhat different from the previously described one is shown by the angle $r \wedge r'$, which, measured on cleavage planes, varies from 73° 10′ to 73° 20′, always exceeding the earlier measurement, and the specific gravity, which is 3.71, distinctly higher than that above quoted.

The indices of refraction were measured by E. S. Larsen by the immersion method. The index ϵ was measured directly, and checked by measuring the highest and the lowest indices on the very perfect cleavage pieces and computing the value of ϵ from the formula for the indicatrix

$$\frac{x^2}{\omega^2} + \frac{y^2}{\epsilon^2} = 1$$

knowing the cleavage angle and, hence, the angle of inclination of the section with the basal plane. The results were: $\omega = 1.817 \pm 0.003$, $\epsilon = 1.595 \pm 0.005$, measured directly; $\epsilon' = 1.699 \pm 0.003$ on the cleavage piece, hence, $\epsilon = 1.594 \pm 0.005$.

The indices of refraction of the rhombohedral carbonates are greatly affected by isomorphous replacements, but as in this case the total replacement is only about 3 per cent and as the calcium and magnesium carbonates, which lower the indices, are in such proportions as to compensate approximately the effect of the iron carbonate, which increases the indices (see below),

[3] DANA, E. S. System of Mineralogy, 6th ed., p. 279, 1892.

this specimen should yield indices almost identical with those of pure rhodochrosite, $MnCO_3$.

The indices of refraction of the Colorado rhodochrosite agree, in fact, closely with the values given by Winchell[4] and ϵ is slightly less than the value given by Ortloff[5] for a specimen containing $MnCO_3$ 93.08, $FeCO_3$ 7.12, $CaCO_3$ 0.27, which would be expected to yield a somewhat high result because of the $FeCO_3$ present. The author (E. S. L.) has measured ω on several other specimens of rhodochrosite, and found the values almost identical in all of them, as shown in Table 1.

TABLE 1

INDICES OF REFRACTION OF RHODOCHROSITE

	LAKE COUNTY, COLO.	CLIMAX, COLO.	OFERNEST, GERMANY	QUOTED, WINCHELL	QUOTED, ORTLOFF	INFERRED
$MnCO_3$.....	95.7 per cent	—	—	—	93.1 per cent	100 per cent
ω	1.817	1.817	1.818	1.82		1.818
ϵ	1.594			1.60	1.597	1.595

Siderite. Two measurements of the indices of refraction of analyzed specimens of fairly pure siderite are available in the literature. The nearly pure $FeCO_3$ from Camborne was found by Hutchinson[6] to have $\omega = 1.8724$, $\epsilon = 1.6338$, while the siderite from Wolfsberg, which is much higher in $MnCO_3$ and $MgCO_3$, is reported by Ortloff[7] to have $\omega = 1.9341$, $\epsilon = 1.6219$. These data are inconsistent; the purer specimen should have the higher indices, since the impurities $CaCO_3$, $MgCO_3$, and $MnCO_3$ all have considerably lower indices than $FeCO_3$. The authors have, therefore, checked the data by measurements on two specimens of recently analyzed siderite. These measurements agree very closely with the data of Hutchinson, so that we regard Ortloff's value for ω as undoubtedly in error, although his value for ϵ is satisfactory.

[4] WINCHELL, N. H., and A. N. Optical Mineralogy, p. 149, 1909.

[5] ORTLOFF, W. *Beitrag zur Kenntnis eutropischer Reihen.* Zeits. phys. Chem., **19**: 215. 1896.

[6] HUTCHINSON, A. *The chemical composition and optical characters of chalybite from Cornwall.* Min. Mag., **13**: 209. 1903.

[7] ORTLOFF, W. Loc. cit., pp. 215–6.

The compositions and indices of refraction of four siderites are given in Table 2 for comparison. The data for the siderite associated with the cryolite from Ivigtut, Greenland, and those of the mineral from the Spokane locality are new. The latter

TABLE 2

INDICES OF REFRACTION OF ANALYZED SPECIMENS OF SIDERITE

	GREENLAND[a]	SPOKANE[b]	CAMBORNE[c]	WOLFSBERG[d]	INFERRED
$FeCO_3$	93.49	93.16	98.43	77.32	100.00
$MnCO_3$	5.16	tr.	1.82	17.04	
$MgCO_3$	0.62	1.83	0.26	5.42	
$CaCO_3$	0.37	5.13	0.18	0.86	
Gangue	1.15				
Sp. gr.	3.94	3.84	3.937		3.96
ω	1.871	1.858	1.8724	[e]1.9341	1.875
ϵ	1.631	1.622	1.6338	1.6219	1.635

[a]Analysis and sp. gr. by E. T. W., optical data by E. S. L. U. S. Nat. Mus. Cat. No. 17571.

[b]Analysis by J. P. Maider, City Chemist of Spokane, made at the instance of Mr. Henry Fair and kindly furnished by Mr. L. P. Gratacap of the American Museum of Natural History of New York; the sp. gr. determination is by Mr. Gratacap, and the optical data by E. S. L.

[c]Hutchinson, loc. cit.

[d]Ortloff, loc. cit.

[e]This value must be in error as pointed out above.

occurrence will be described in a forthcoming number of the *American Mineralogist*. The probable error in the indices of refraction for these two new measurements does not exceed ± 0.005.

GENETICS.—*The rôle of selection in evolution.*[1] W. E. CASTLE, Bussey Institution.

Up to the year 1900 those who believed in organic evolution almost without exception believed in selection as its efficient cause. Then came a period of doubt, inaugurated by DeVries' Mutation Theory and strongly supported by Johannsen's Pure Line Theory. In the minds of many biologists at the present time selection is an obsolete agency in evolution and an adequate explanation of evolution is to be found only in mutation and pure lines. I believe this to be a mistaken view, not because mutation and pure lines are false, but because their applicability is very limited as compared with the broad field of organic evolution. To universalize them is to hide the world by holding a small object close to the eye. For even if we concede the strongest possible claim for mutation as an agency in evolution, viz, that it produces all new and heritable variations, it is still unable to produce evolution without the aid of selection. The production of new variations produces no racial change unless those variations persist, but their persistence depends wholly upon selection. This is admitted by DeVries, the author of the mutation theory, but overlooked by many of those who have adopted the term mutation, as a scientific shibboleth.

But it is idle to enter upon a discussion of either selection or mutation without carefully defining these terms, since both are often used quite ambiguously, the latter in particular being used in several different senses, and so being a cause of misunderstanding where no genuine difference of view exists.

Ever since DeVries' original attack in 1900, it has become increasingly common among biologists to refer with disrespect to "Darwinian selection." But Darwin understood by selection any agency which would cause one organism to survive rather than another, and it is not clear that any theory of evolution can dispense with such an agency. Since more organisms are born than can survive, some must perish. In a state of

[1] A lecture delivered before the Washington Academy of Sciences, April 13, 1917.

nature, that is, in a state of affairs not actively controlled by man, those creatures survive which are best adapted to their surroundings. This is what Darwin meant by "natural selection." Among organisms under the immediate control of man, as the cultivated plants and domesticated animals, where the determination of what individuals shall become parents rests with man, Darwin recognized the occurrence of "artificial selection."

Any legitimate attack on Darwin's views of selection must deal either with natural selection or with artificial selection. But when "Darwinian selection" is mentioned as a term of reproach, the attack is really directed neither against natural selection nor against artificial selection, nor against any other conceivable form of selection, but against one of Darwin's views as to the nature of variability. Darwin recognized two sorts of heritable variations, (1) those which are purely quantitative, plus or minus, as compared with the prevailing racial condition, and (2) those which are wholly different from the prevailing condition. The former we may call "fluctuations," adopting the convenient term of DeVries. The latter Darwin often called "sports." Bateson has called them discontinuous variations, and DeVries calls them mutations. Darwin believed that evolution might result either from the systematic and repeated selection of fluctuations or from the propagation of sports. DeVries doubts whether the systematic selection of fluctuations amounts to much in an evolutionary way, and Johannsen has denied to it any evolutionary effect whatever, on the ground that fluctuations are not inherited. Darwin assigned to the selection of fluctuations a major part in evolution, DeVries assigned to it a minor part, and Johannsen allows it no part in evolution. As regards sports, Darwin assigned to their selection a minor part in evolution (chiefly among cultivated plants and domestic animals); DeVries ascribed to a particular kind of sports (his "mutations") a major part in evolution; and Johannsen ascribes an exclusive part in evolution to a type of variation which would include both Darwin's sports and DeVries' mutations and then some. Johannsen has indeed made a new classification

of variations which is both logical and sound, but which has resulted in some confusion owing to efforts to combine it with earlier classifications. He classifies variations into those which are inherited (genotypic) and those which are not inherited (phenotypic). No objection can be made to this classification except that it raises new difficulties and solves none. For how is one to distinguish a phenotypic from a genotypic variation? Only by trying them out. A variation which is inherited is genotypic; one which is not inherited is phenotypic. Since there is no other way then actual experiment by which to distinguish genotypic from phenotypic variations, we acquire only a new set of synonyms for inherited and non-inherited, a thing for which there was no urgent need.

Attempts to combine the classifications of variations made respectively by Darwin, by DeVries, and by Johannsen have resulted in serious confusion which is largely responsible for the apparently contradictory views held at present concerning selection. There really is no diversity of view concerning selection but only concerning the nature of the material that it acts upon (viz., variations).

To complicate the situation still farther we have the discovery of Mendelian unit-characters which introduces a new uncertainty. Are these unit-characters fluctuations or sports? Do they arise solely by mutation or also by the cumulation of fluctuations? These are vital but perplexing questions. As matters stand concerning terminology, we have the term "sport," introduced by Darwin but now largely discarded, meaning any discontinuous, striking, suddenly appearing variation, known to be strongly inherited. Some of the examples cited by Darwin, such as the Ancon sheep, obviously involve Mendelian unit-characters.

The term mutant as used by DeVries signifies much the same as Darwin's term sport but involves a particular conception of the circumstances and manner of its origin which is not involved in Darwin's term. Some of DeVries' mutants of the evening primrose involve Mendelian unit-characters, as for example his dwarf mutant (*nanella*), while others such as *gigas* do not.

The latter involves a double representation of every chromosome in the cell nucleus; the *lata* mutant involves the presence of a single extra chromosome. What chromosome changes, if any, are involved in other of DeVries' mutants which do not Mendelize is unknown. Morgan has shown that in Drosophila a unit-character change almost certainly involves a change in a definitely localized part of a single chromosome. But he applies the term mutation to each unit-character variation of Drosophila, of which he has observed over a hundred. Some of these are not at all striking, involving only a slight change in the shape, size, venation, or carriage of the wing, which might easily be overlooked by the ordinary observer. Many of them also fluctuate. Hence it is obvious that Morgan's use of the term mutation is very different from that of DeVries, its originator. To Morgan, mutation as illustrated in Drosophila is simply change by a unit-character. With this conception of mutation, Morgan attempts to combine the genotype conception of Johannsen. He regards unit-character variations as the only kind of genotypic variations and these as fluctuating (if at all) only through the interaction of other unit-characters, each one by itself being incapable of fluctuation.

It will be observed that as regards the term mutation, we have a very confused state of terminology which results in much discussion at cross-purposes, because persons using the same term have different things in mind.

But in this discussion, however confused its terminology, there are really involved two contrasted sets of general ideas, two alternative lines of explanation of evolutionary change, one favored by Darwin, the other offered as a substitute by DeVries and accepted by Johannsen and Morgan. We may briefly outline them as follows:

DARWIN	DEVRIES
1. New types are for the most part created gradually.	1. New types are created only abruptly.
2. New types are for the most part plastic.	2. New types are fully stable.
3. One evolutionary change follows upon and is made possible by another.	3. One evolutionary change has no necessary relation to another.

4. Natural selection determines what classes of variations shall survive and, in consequence, what shall be the variable material subjected to selection in the next generation.	4. Natural selection determines only what classes of variations shall survive, and exercises no influence on the subsequent variability of the race.
5. The further evolution of our domestic animals and cultivated plants (and of man himself) is to some extent controllable because we can by selection influence the variability of later generations.	5. Evolution is beyond our control except as we discover and isolate variations.

These two sets of contrasted views remind us somewhat of the theological ideas of free-will and predestination respectively, which resemblance will account for the preferences of some biologists but will not prove which is right and which is wrong. This is wholly a matter for evidence. But what conclusion one reaches will depend much upon what sort of evidence he studies. Paleontology, geographical distribution, classification, and experimental breeding, all present evidence which must be weighed before a safe verdict can be framed.

Paleontology, the study of the actual historical records of evolution found in the rocks, indicates in the case of the most complete series of fossils, as for example of the horse, the camel, and the rhinoceros, that the evolution of these types was a gradual process, though of course their appearance in particular continents may have been abrupt, owing to migration. It indicates further that these and other types, when they first appeared, were plastic, and generalized and varied in many different ways, most of the variations later disappearing and leaving only a favored few lines of specialized survivors. It shows too that one variation paved the way to another. The five-toed horse first becomes four-toed, then three-toed, then one-toed. There is no mutation from five-toed to one-toed, nor from the size of a fox to that of a draft horse. As to natural selection, paleontology is silent, because the causes of extinction are unknown. But on the whole the weighty evidence of pale-

ontology supports the view that evolution as an age long process has been gradual and progressive, not abrupt and unguided.

Geographical distribution and classification favor the same idea. Related species are most often found in contiguous territory. Species not closely related are commonly far separated in space or have been long separated in time. Nothing indicates that of two related species one has sprung suddenly from the other. They are not distinguished from each other, as a sport from its parent form, by some single Mendelian unit-character, but they differ morphologically by a large number of quantitative differences, and physiologically they differ to such an extent that frequently they will not interbreed when brought together even though their morphological differences are small, or they will produce sterile hybrids, or those of a blended, intermediate character. In all these particulars they show that they have not diverged by mutation, either in the sense of De Vries or in that of Morgan, but by a gradual progressive process.

Finally we come to the evidence from experimental breeding. Some say that this is the only legitimate evidence as regards the method of evolution because it alone is experimental. I should be the last to deny its importance because I have devoted much time to its pursuit in the firm conviction that it could yield valuable evidence, but frankness compels one to admit that this method of study, like all the others, has limitations of its own. The experimental breeder can study a few successive generations with an intensiveness that is possible by no other method, but his glimpses of evolution at work are momentary as compared with the studies of the paleontologist. He can witness the production of new sorts but it is doubtful whether any man has witnessed the contemporary production of a new species, in the sense of the paleontologist and the student of geographical distribution. Evolution is undoubtedly at work all the time, but the breeder is not always in a position to say just what is happening. It takes a succession of views in a motion picture to show what objects are stationary and what are moving, and the breeder's view of the evolutionary process often fails to reveal which is which.

On the other hand, the experimental breeder, though he lacks perspective, is dealing with the actual material concerned in organic evolution. He can see and handle it and observe it change under his hands, as no other student of evolution can. But the changes which he observes taking place must be correctly interpreted if valid conclusions are to be reached concerning the general process of evolution. At present experimental breeders are divided in their views. The very same facts are interpreted by some as indicating an orderly progress toward definite end results, and by others as nothing but haphazard unrelated chance occurrences. Just now the latter method of interpretation, embodied in the mutation theory, is very popular among experimental breeders, although it has few adherents among students of paleontology, classification, or geographical distribution.

The principal tools of the experimental breeder are hybridization and selection. All are agreed that hybridization (using the term in its broadest sense) is, in the hands of the breeder, a very potent agency in producing variability, upon which selection may then be brought to bear for the production of new or modified types. Lotsy even goes so far as to suggest that all genetic variability is the result of hybridization, but this is flatly disproved by observations of Johannsen who reports the occurrence of mutations in genotypically pure lines of beans, as also by the remarkable series of variations observed by Morgan in an inbred race of Drosophila.

As regards the action of selection, the most widely divergent views are held by experimental breeders. The mutationists hold that it can do nothing but isolate variations which may sporadically put in an appearance or which may by hybridization be brought together into new combinations. Those who differ from them, and whom they call selectionists, maintain that selection can accomplish more than the mere isolation of variations because it can, by a series of selections, influence further variability. I confess myself an adherent of this at present somewhat unpopular view. I hold it, not because Darwin held it, nor merely because paleontologists, systematists,

and students of geographical distribution in general favor it, nor because DeVries and Johannsen have attacked it, but because the facts of experimental breeding, as I understand them, prove it.

For DeVries may be claimed the merit of having first systematically set about testing the effects of selection by actual experiment. His methodical selections for many years in succession of maize, buttercups, striped flowers, and four-leaved clover will long be remembered, but they fall far short of conclusiveness because they were not continued long enough to show whether selection had attained all that was attainable under existing variability or whether further variation in the direction of selection would occur, and because DeVries' cultures were not sufficiently guarded from hybridization which might conceivably influence the result. These necessary precautions were fully met by Johannsen, who in the case of beans, which are self fertilizing but show fluctuating variation in the size of the seed, proved that selection generation after generation in a particular direction may be without result, so far as any change in average seed size is concerned. Cases of this sort involve "pure lines," those which are devoid of genetic variation to any appreciable extent in the character studied, size of seed. But in other cases, as where Johannsen made his size selections from a field crop harvested from many different plants, he found that average size was influenced by selection, which he reasonably explains on the ground that the material from which selection was made consisted of a mixture of pure lines genetically distinct. The correctness of Johannsen's conclusion has been repeatedly verified in the case of other self-fertilizing plants such as wheat and oats. Attempts were at once made to generalize Johannsen's brilliant demonstration of the principle of pure lines in the following ways:

1. Since a line of beans long self-fertilized is devoid of genetic variation in seed size, self-fertilization, if long enough continued, will produce lines genetically pure as regards *all* characters. Selection can not bring about modification of such pure lines. In respect to this generalization it may be said that it remains to

be shown that beans are as devoid of genetic variation in other particulars as they are in seed size, which the argument assumes to be true. Further, if various pure lines of beans have come into existence by an evolutionary process (descent from a common ancestor, with modification) it is evident that differences must have arisen which did not originally exist. Suppose we grant Johannsen's (unproved) contention that such differences arise by mutation only. If they arise in this way (or in any other way whatsoever), selection can isolate them, and if they are at all frequent in occurrence, selection can be continuously effective in producing racial changes. It would all come down then to a question of how frequent mutations are in a particular case. Johannsen concedes their occurrence even in beans. It may well be that in some organisms they are commoner than in others and that in beans they happen to be particularly infrequent.

2. Johannsen's case has been further generalized to include *all* self-fertilizing organisms, which are supposed to fall automatically into pure lines (i.e., those devoid of genetic variation) as regards all characters. This too requires proof, but has been found to be a safe working hypothesis in the case of cereals, tobacco, peas, and other economic crops, in the attempted improvement of which selection of fluctuations, unless preceded by hybridization may be regarded as a waste of time, for the reason that genetic variation is so rare under continuous self-fertilization that the breeder will obtain variation much more quickly by resorting to hybridization.

3. Further, it has been argued that if cross fertilization alone interferes with the automatic production of pure lines, then any organism which dispenses with fertilization altogether, reproducing asexually, must *ipso facto* constitute a pure line. Jennings sought to test out this conclusion by experiment. He selected size variations in Paramecium which reproduces by fission, with success in the case of mass cultures of unknown origin, but without success in the case of cultures made from single individuals. This was regarded as strong confirmation of the pure line principle until Calkins and Gregory, repeating the experiment on ex-conjugants, were unable to support it. Then

Jennings, selecting a new species of Protozoa, more favorable for precise quantitative observation, also obtained a different result. He now found that among the observed fluctuations in size, those of a genetic character were included, so that by repeated selection races could be produced which were progressively larger or smaller, rougher or smoother. This is fully in harmony with the observations of Stout who found that variations in Coleus arising in asexual propagation were capable of further propagation. It also harmonizes with the observation of Shamel as regards the occurrence in citrous fruits of bud variations which are important enough to warrant propagation in economic work; and further, with Winkler's clear demonstration of the occurrence in the tomato and the night-shade of *gigas* like mutations, arising first in single somatic cells, which asexually propagated produce entire plants of a new type which then are self-perpetuating by seed. We also have the observations of East that in the asexual propagation of the potato occasional bud variations may occur which are similar in nature to unit-character variations in reproduction by seed. It is accordingly clear that the pure-line principle does not apply without exception to asexually reproducing organisms any more than it does to self-fertilizing ones. It is true, however, that genetic variations are much less common among such organisms than among those produced by cross-fertilization. Herein lies the justification of present agricultural practice in the breeding of self-fertilized cereals, and of horticultural practice in the propagation by grafts, runners, layers, etc., of superior individual plants.

4. Attempts to extend the pure line principle to organisms which are not self-fertilizing (and this includes all the domestic animals and many cultivated plants) have met with small success. Morgan indeed assumes that it applies to his races of Drosophila up to a certain point, the point at which mutation begins, but the mutations which he recognizes are so numerous, so minute in many cases, and so fluctuating in others, that it becomes a question whether his "mutations" are not just ordinary heritable variations. Morgan would undoubtedly admit this since he claims that *all* heritable variations arise as mutations,

but this is simply juggling with names, giving a new meaning to the word mutation in order to justify a sweeping generalization otherwise untenable.

The test of a pure line is its freedom from any genetic variation, so that selection cannot modify the racial mean as regards any character. As soon as any race of animals or plants changes in response to selection, it must be forthwith excluded from the category of pure lines. The consequence is that no case of a pure line among animals has yet been demonstrated. Nevertheless the "*principle* of the pure line" is in some way or other supposed by the followers of Johannsen to confer on even the higher animals a limited liability to modification in consequence of selection.

Thus Pearl having been entrusted in 1908 with a selection experiment for increase of egg production in Plymouth Rock fowls, an experiment which had already been in progress for nine years, decided after a study of the records kept by his predecessor that no improvement whatever had up to that time been made and further that none probably could be made since individual wild birds probably lay, under favorable conditions, as many eggs as their best tame relatives. This reasoning was strictly in accordance with the "pure line principle" and was in fact based on it.

Later by changing somewhat the basis of selection, so as to rank his animals on the basis of their progenies' performance as well as their own, Pearl found that he could considerably increase the flock average. Yet he still maintains that he has only *more* good birds not *better* ones, than when the experiment began, and in loyalty to the pure line principle he has no expectation of obtaining better ones in the future, since he already has and has had all along the *ne plus ultra* sort. One less devoted than Pearl to a generalization of the pure line doctrine would continue hopefully the effort to produce a *better* fowl as well as to produce more good ones. For the function of egg-production admittedly depends upon many physiological factors (as well as several external ones). These physiological factors must many of them be independently variable and to some extent independ-

ently heritable. Variation in one or more of these factors (by mutation or otherwise) would undoubtedly influence the total productiveness, and the probability of the occurrence of a mutation would increase with the number of factors involved. So that even one formally committed to the pure line doctrine, but admitting as Johannsen does that mutations do occasionally occur in pure lines, might hopefully continue to look for improvement in the standard of egg-production. No other method of detecting and utilizing a favorable variation, when it does occur, can be suggested than the very method of methodical and persistent selection against which the pure line advocates direct such vigorous attacks.

Morgan is a formal adherent of the pure line doctrine, but pragmatically a selectionist for he admits the great progress made in the improvement of domestic animals and plants by selection, and even that his own mutants of Drosophila fluctuate and yield modified forms in response to methodical selection, as for example the bar-eyed mutant, subjected with success to plus and minus selection by Zeleny. But he attempts to explain these results in harmony with the pure line principle by assuming that, whenever a modification is observed in any character, this is due to a mutation, and if a graded series of modifications is obtained, as in the plus and minus selected bar-eyed Drosophila, this is due to a multiplicity of mutating factors whose action on the chief factor concerned is purely incidental. On this view, however, the attainment of a completely homozygous condition on the part of all factors (if all are indeed Mendelian) would put an end to genetic variability, and selection would then cease to produce effects. Such a completely stable condition has, however, rarely been demonstrated. One case is reported by MacDowell, that of a race of Drosophila with an extra number of thoracic bristles. The average number of bristles was increased by selection for six generations but then showed no further increase and could not subsequently be changed either upward or downward by further selection. The race had apparently become a "pure line" for bristle number.

In the case of certain characters in guinea-pigs I have repeatedly attempted modification of a racial character by selection within an inbred race, without success. Thus a very dark form of Himalayan albino, after a certain amount of improvement by selection, could not be further darkened to any appreciable extent. A race selected simultaneously for large size and for small size showed so little change that the experiment was abandoned after a few generations. No indication was forthcoming that we could thus ever approach in size either the small wild *Cavia Cutleri* of Peru, or the large races of guinea-pig kept in captivity by the natives of the same region. Yet evolution had in some way evidently produced these divergent conditions from a single original source. The changes were probably too slow to be observable in the life time of one observer.

On the other hand, certain characters of guinea-pigs, rabbits, and rats have been found to respond readily to selection in a particular direction. This is notably true of color patterns which involve white spotting. A selection experiment with hooded rats selected simultaneously in plus and minus directions has produced one race which is black all over except a white patch of variable size underneath, and another race which is white all over except for the top of the head and the back of the neck, which are black. The races do not overlap at all and have not done so for many generations, though they still continue to diverge from each other as a result of continued selection.

In similar experiments with Dutch marked rabbits it has been found possible by selection to increase or decrease the amount of white at will. In a series of such rabbits ranging from nearly all black to nearly all white, stages far enough apart to be certainly identifiable behave as Mendelian allelomorphs in crosses, but regularly emerge from such crosses in a slightly modified form, the whiter stages having been darkened and conversely the darker stages whitened. The principle of the pure line manifestly does not apply to these cases. White spotting is apparently a character which from its nature fluctuates constantly, such fluctuations having, to some extent at least, a genetic basis, since continuous selection invariably produces a

modified race. Even in wild species, such as the skunks, white-spotting is manifestly a variable character, which no doubt will respond to the selective efforts of our skunk farmers, who desire an all-black race. Why white-spotting should be a less stable character genetically than some others, it is impossible to say, but the fact is beyond question. Morgan has suggested that in general the genetic basis of a Mendelian character may be a single molecule, and gives this as a reason for believing in its constancy. But white spotting can hardly fall in with this conception. It seems to me more probably due to a quantitative deficiency in the germ of some substance which normally finds its way into all epidermal cells of the body and which is responsible for the development in them of melanin pigment. Greater and greater deficiencies of this substance cause more and more extensive white areas.

Complete or total albinism behaves very differently. It results from a complete change in some color factor which may well be a simple molecule since it appears to be incapable either of contamination in crosses or of modification under selection. Nevertheless the color factor (molecule or whatever it may be) evidently is not so simple but that it can assume at least four mutually allelomorphic forms, as shown for the guinea-pig by Wright, a like number of allelomorphs, though not their exact equivalents, being known also in the rabbit.

As regards the agouti factor in mice, rabbits, and guinea-pigs, this too may assume several different allelomorphic conditions, though it is not certain that any one of these fluctuates or can be modified other than by associating with it unrelated genetic factors.

The divergent conclusions which students of genetics have reached concerning the stability of Mendelian genes and the consequent effects of selection for their modification are probably due in part to the particular choices which they have made of test cases. A study of albinism alone would lead one to believe in the fixity and constancy of Mendelian genes and the impossibility of modifying them by selection. A study of white spotting leaves one with the unshakable conviction that this form of

gene is plastic and yields readily to selection. Where only genes of the former sort are involved, the principle of the pure line is applicable; where genes of the latter sort are involved, it is not applicable. The divergent results obtained by Jennings when dealing with Paramecium and when dealing with Difflugia indicate that among asexually reproducing organisms, also, genes are involved, some of which are stable, some of which are not. Accordingly, what conclusion we reach as to the applicability of the pure line theory in the breeding of animals and plants will depend upon how common we find stable and plastic genes respectively to be, and in what sorts of variations they are involved.

My own opinion, based upon a study through many years of a variety of inherited characters in the smaller mammals, inclines to the view that in such animals very few characters can safely be referred to the agency of perfectly stable genes. Even in color characters, probably the simplest as well as the most studied of inherited characters, there is much fluctuation which yields substantial results to selection by the discriminating breeder. The yellows are not all of one shade, nor the blacks of equal depth. The golden yellow of the Guernsey cow is very different from the fawn of the Jersey or the dark red of the Devon. Yet all are yellows, allelomorphs of black, but each is selected for a different standard to which the breeder must adhere very carefully in his selections, if he wishes to win prizes or sell breeding stock.

When it comes to size and shape and that consistent interrelation of parts which the breeder calls "conformation," stable genes cannot be detected. Crosses produce blends as regards size and shape, and conformation is completely dissipated by a cross. That is why the breeder is so reluctant to resort to an outcross unless he is engaged merely in meat or wool production and is not attempting to breed to a type. *Aside from color there are very few valued economic characters in our domestic animals which are not inherited after the manner of blends.*

Weight of carcass, quality of wool, milk production in cattle, egg production in fowls—all these are blending characters which

in later generations show either no segregation or imperfect segregation (fowls, Pearl[2]). I do not say that in these cases no Mendelian inheritance is involved, but merely that no stable genes are in evidence, nothing that would preclude the probable effective use of selection in maintaining or raising breed standards.

If we turn from the breeding of animals, in which manifestly the pure line principle has little applicability, to the breeding of plants other than those which are self-fertilized, we again find that this principle has a very limited applicability. Probably the most valuable open pollinated field crop in cultivation is maize. But a pure line of maize is not known to exist. An experiment which should have lead to the production of pure lines, if such a thing were attainable in maize, has been in progress at the University of Illinois for the past twenty years. Selection has been made for increased and for decreased protein content of the grain, and also for both increased and decreased oil-content, with the result that steady progress in the direction of selection has in every case been made. The high protein strain now contains twice as much protein as the low protein strain; and the high oil strain contains four times as much oil as the low oil strain. The divergence of the selected lines from each other is not now as rapid as at first but it still continues steadily, with no indication that it is soon to cease, as must be the case if only stable genes were involved.

Those characters in maize which directly affect the yield, such as size of plant, or of the grain which it bears, are blending in inheritance and show imperfect segregation subsequently. They are probably all of them quite as amenable to selection as the oil content and protein content of the seed, experimented upon in Illinois.

[2] It is true that Pearl (1912) has described fecundity in fowls as "typically Mendelian" in heredity but his figures show that in crosses between Barred Rocks and Cornish Indian Games, the average fecundity of the F_1 birds is in both the reciprocal crosses intermediate between that of the respective parent races though nearer the racial average of the sire, which supports his contention that a sex-linked gene is involved, but shows also that this is not the only factor involved. Back-crosses of F_1 of both sexes with the pure races give evidence of further blending (or imperfect segregation) on the part of the non-sex-linked factor or factors.

Finally, as evidence that even in self-fertilized plants the pure line principle may be inapplicable because of the existence of genes which are plastic, let me cite a very extensive and carefully executed piece of work on garden peas done by Hoshino. He studied the behavior of flowering time, and showed that its inheritance involves a Mendelian gene coupled with flower color (white or red). The inheritance of flowering time is intermediate, but F_1 is closer to the late than to the early parent in this character. Segregation is imperfect in F_2 with a range practically all the way from the early to the late parent, but not transgressing this range. F_3 and F_4 families from self-fertilized parents are in many cases quite variable but others are no more variable than the pure parental varieties and so may be treated as practically "constant." A study of the average flowering time of each of the 230 "constant" F_4 families shows that these fall into three main groups, some falling into a *modified early* group, not quite so early as the early parent, others falling into a *modified late* group, not quite so late as the original late parent, but most of all falling into an intermediate group occupying the region midway between the parent varieties in flowering time. Considered all together, the F_4 families "constant" for flowering time form an almost uninterrupted series of conditions connecting the respective parental conditions seen in the early flowering and in the late flowering race.

These observations show the existence of a gene for flowering time in peas which is decidedly plastic. That a gene actually exists is shown by its coupling with flower color. That it is plastic is shown by the fact that it emerges from the cross nearly always in a modified form. When the possibility of modification has been continued as long as the F_4 generation, the majority of the "constant" families are found in the intermediate or middle group. The plasticity is here shown in a tendency of the contrasted genes to blend into one of intermediate character. It is also shown in data given by Hoshino as to flowering time in parent individuals and their offspring in the late flowering variety. Although this variety is treated by Hoshino as a "pure line," it is evident that within this line itself the later flowering in-

dividuals have later flowering offspring and *vice versa*. In other words selection within this supposed "pure line" is evidently effective. Accordingly either the gene here involved is plastic or the supposed pure line is not pure.

From the various lines of evidence which have been cited (and I might have cited many more) it is clear that the pure line principle, valid as a working hypothesis for seed size in beans and for certain morphological characters in self-fertilized cereals, does not fit in with the observed facts as regards the effects of selection in the majority of the domesticated animals and cultivated plants, nor even with the behavior of certain characters in self-fertilized plants and asexually propagated animals. In the case of such characters as white spotting in mammals, it is evident that a change in the mean of the character in a particular direction in consequence of selection actually displaces in the direction of selection the center of gravity of variation, so that in a very true sense selection makes possible further variation in that same direction. The same is probably true as regards protein content and oil content in the Illinois corn experiment. It is doubtful whether, outside of that particular experiment, maize with as high a protein content as 15 per cent has ever been observed, or maize with as high an oil content as 8.5 per cent. It is not then a misuse of terms to say that the selection has in this case been the *cause* of further variation in the direction of selection and so an agency in the progressive evolution of a new type.

If this is true concerning a single character under experimental study for a period of twenty generations, may it not also be true of entire organisms and groups of organisms subjected to keen competition with all other organisms in a struggle for existence which has continued for millions of generations? If there are characters which are plastic under artificial selection, why need we be skeptical about the plasticity of organisms subjected to natural selection? If artificial selection can, in the brief span of a man's life time, mould a character steadily in a particular direction, why may not natural selection in unlimited time also cause progressive evolution in directions useful to the

organism? I am not ready to say that natural selection is proved as the method *par excellence* of evolution, but I am not ready to abandon it as the most reasonable explanation of evolution until something better supported than the mutation theory is offered as a substitute for it. At the same time the fact should be emphasized that biology has benefited greatly from the investigation and the discussion initiated by the mutation theory. Even though the mutation theory cannot be accepted as a general theory of evolution it has done us great good in dispelling or clarifying the hazy notions which formerly existed as to what natural selection could accomplish. Selection, whether natural or artificial, is, as the mutation theory rightly holds, primarily an agency for the elimination of variations, not for their production. It can only act on variations actually existing, and while it can, I believe, continue and extend variation already initiated by shifting in the direction of selection the center of gravity of variation, it cannot initiate new lines of variation. It cannot change a vertebrate into something else nor something else into a vertebrate. It is limited to the modification of existing types of organisms, and to their modification in directions in which they show a tendency spontaneously to vary.

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ZOOLOGY.—*A revision of the recent genera of the crinoid family Bourgueticrinidae, with the description of a new genus.*[1] AUSTIN H. CLARK, National Museum.

Of all the families of stalked crinoids represented in the recent seas the Bourgueticrinidae is the most universally distributed, occurring in all the oceans and ranging from 112 meters (62 fathoms) or less to a depth of 4842 meters (2690 fathoms), the greatest depth at which stalked crinoids have been found.

The genus *Metacrinus*, now dominant in the East Indies and ranging from southern Australia and Tasmania to southern Japan, includes about 25 species; but aside from this genus the Bourgueticrinidae with its 30 species is more numerously represented than all the other stalked families together.

Heretofore the recent species of Bourgueticrinidae have been grouped in two genera, *Rhizocrinus* and *Bathycrinus*, though it has long been evident that such a disposition was far from satisfactory; especially has this been the case since the discovery of the species of *Monachocrinus*, which have the general appearance of the species of one of the genera (*Bathycrinus*), but the detailed structure of those of the other (*Rhizocrinus*).

The recent species of the Bourgueticrinidae fall into six natural groups, which are differentiated as shown in the following key:

Key to the Recent Genera belonging to the Family Bourgueticrinidae

a^1 The third, sixth, and ninth brachials (the fifth, eighth, and eleventh ossicles from the radials) have a muscular articulation on either end; basals always fused into a solid ring which is broader than long, cylindrical or short truncated-conical; ten arms

b^1 The distal edges of the brachials are produced, overlapping the bases of the succeeding brachials, so that the dorsal profile of the arms is serrate; the IBr_2 (axillary) is markedly broader than long, distinctly shorter than the IBr_1 *Bathycrinus.*

b^2 The distal edges of the brachials are not produced, so that the dorsal profile of the arms is smooth; the IBr_2 (axillary) is little, if any, broader than long, and is about as long as the IBr_1 *Ilycrinus.*

[1] Published with the permission of the Secretary of the Smithsonian Institution.

a^2 All the post-radial ossicles are united in pairs by non-muscular articulation; the basals are separate, or are fused into a solid ring which is truncated conical, always longer than broad

b^1 Ten (or twelve) arms, the second post-radial ossicle being axillary

Monachocrinus.

b^2 Five undivided arms

c^1 No sutures visible between the basals; the basals are solidly welded into a single conical ossicle

Rhizocrinus.

c^2 Basals always separated by distinct sutures

d^1 Column comparatively slender, the longer columnals being at least twice as long as broad; calyx distinctly conical

Bythocrinus.

d^2 Column very stout, the longer columnals being but little longer than broad; calyx nearly or quite cylindrical

Democrinus.

Bathycrinus Wyville Thomson.

Bathycrinus WYVILLE THOMSON, Proc. Roy. Soc. Edinburgh, **7,** 1872, p. 772 (genotype *B. gracilis,* sp. nov.).

Geographical Range.—Mid-equatorial Atlantic and northwestward to the coast of Virginia and Maryland (as far as 38° 20′ N. lat.), and northward to the latitude of the northern part of the Bay of Biscay (47° 38′ N. lat.); eastern part of the Bay of Bengal, east of the northern end of Sumatra; mid-Pacific between Oceania and America (from 0° 3′.4 to 9° 57′ N. lat.).

Bathymetrical Range.—From 1629 to 4842 meters (905 to 2690 fathoms).

Thermal Range.—From 34°.3 to 36°.8 Fahr. (all but one of the records are between 36°.5 and 36°.8).

Included Species.—*Bathycrinus aldrichianus* Wyville Thomson, *Bathycrinus equatorialis* A. H. Clark, *Bathycrinus gracilis* Wyville Thomson, *Bathycrinus pacificus* A. H. Clark, *Bathycrinus serratus* A. H. Clark, *Bathycrinus sibogae* A. H. Clark, and *Bathycrinus woodmasoni* A. H. Clark.

Ilycrinus Danielssen and Koren.

Ilycrinus DANIELSSEN and KOREN, Nyt Magasin for Naturvidenskaberne, **23,** 1877, 3 die Hefte, p. 45 (genotype *I. carpenterii,* sp. nov.)

Pterocrinus (Wyville Thomson, MS.) P. H. CARPENTER, "Challenger" Reports, Zoology, **11,** 1884, p. 242, 243 (genotype *Bathycrinus aldrichianus* P. H. Carpenter, 1884 [not of Wyville Thomson, 1878] = *Bathycrinus australis* A. H. Clark, 1907).

Geographical Range.—Antarctic regions, from west of the Crozet Islands to Enderby Land; extreme north Pacific from the Commander

Islands to between Sitka and the Columbia River; the cold deep water between Norway and Iceland, and northward.

Bathymetrical Range.—From 1337 to 4636 meters (743 to 2575 fathoms).

Thermal Range.—From 30°.9 Fahr. and −1°.6C. to 36°.6. Fahr.

Included Species.—*Ilycrinus australis* (A. H. Clark), *Ilycrinus carpenterii* Danielssen and Koren, and *Ilycrinus complanatus* (A. H. Clark).

Monachocrinus, gen. nov.

Monachocrinus A. H. Clark, Eastman's translation of Zittel's Palaeontology, 1913, p. 230.—A. H. Clark, Internationale Revue gesamten Hydrobiologie und Hydrographie, 1914, p. 7.—A. H. Clark, Die Crinoiden der Antarktis, 1915, pp. 125, 152, 182.—A. H. Clark, Amer. Journ. Sci. and Arts, **40,** 1915, p. 61. A. H. Clark, Smiths. Miscell. Coll., **65,** 1915, No. 10, p. 11.—A. H. Clark, Amer. Naturalist **49,** 1915, pp. 525, 526, 527, 542.

Diagnosis.—A genus of Bourgueticrinidae in which the arms divide once, on the second post-radial ossicle (being ten or twelve in number); all the post-radial ossicles are united in pairs by nonmuscular articulation; and the basals are separate, or are fused into a solid ring which is truncated conical, always longer than broad.

Geographical Range.—Caribbean Sea to the Azores and Morocco, and northwestward to southwest of Iceland; the Bay of Bengal; the vicinity of Banda and Celébes.

Bathymetrical Range.—From 1236 to 4255 meters (687 to 2419 fathoms).

Thermal Range.—The only two records, both in the Atlantic, are 3°.0C. and 40°.0 Fahr.

Included Species.—*Monachocrinus caribbeus* (A. H. Clark), *Monachocrinus minimus* (Döderlein), *Monachocrinus paradoxus* (A. H. Clark), *Monachocrinus perrieri* (Kœhler and Vaney), *Monachocrinus poculum* (Döderlein), *Monachocrinus recuperatus* (Perrier), and *Monachocrinus sexradiatus,* sp. nov.

Genotype.—*Monachocrinus sexradiatus,* sp. nov.

Monachocrinus sexradiatus, sp. nov.

Description.—The basals are anchylosed into a solid funnel-shaped ring which is rather more than twice as long as broad basally. In lateral view the sides of this funnel are almost parallel in the proximal half, but in the distal half they gradually diverge so that their final direction in relation to each other is the same as that of the two sides of the radial circlet, with which they merge without any deflection.

The radial funnel is composed of six similar radials of equal size, and is about twice as high as broad at the base; in profile it is seen to

be very slightly, almost imperceptibly, concave in the proximal half, becoming slightly convex in the distal; though the proximal end of the radial funnel is circular in outline, each radial distally gradually becomes convex dorsally so that in a dorsal view the distal end appears in outline as a hexagon, with rounded angles; the distance from the summit of the column to the distal edge of the radial circlet is 2 mm.

The IBr_1 are very long, about twice as long as the proximal width, with straight, slightly and regularly diverging sides.

The IBr_2 (axillaries) are little more than half as long as the IBr_1, considerably broader than long, with a blunt distal angle.

Both the ossicles of the IBr series have a low obscure broadly rounded median convexity, carrying forward the convexity of the distal portion of the radials, and sharp straight sides; the IBr_2 has a shallow pit just within the blunt distal angle.

The twelve arms are slender and narrow, with a smooth dorsal profile, the distal borders of the brachials not being produced, and measure 28 mm. in length from the distal edge of the radials; the brachials are all united in pairs by non-muscular articulations; the first brachial is slightly trapezoidal, longer outwardly than inwardly, broader basally than distally, half again as long as broad distally; the second brachial is not much more than half as large.

The first pinnule occurs on from the tenth to the fourteenth brachial, usually on the tenth.

The portion of the column attached to the crown is 22 mm. in length and consists of forty-three segments, of which the first fifteen are short and discoidal and the six following broader than long; the last five or six segments of the column as preserved are about four times as long as broad with very slightly, almost imperceptibly, enlarged ends, and a similarly almost imperceptible median raised girdle.

Another specimen, with the arms 25 mm. long, differs in having the five basals, which are of unequal size, separated from each other by sutures.

Locality.—Southwest of Iceland (61° 44′ N. lat., 30° 29′ W. long.).

Depth.—1135 fathoms.

Bottom temperature.—3°.0C.

Rhizocrinus M. Sars.

Rhizocrinus M. Sars, Forhandl. Videnskab. Selsk., 1864, p. 127 (genotype *Rh. lofotensis*, sp. nov.).

Geographical Range.—North Atlantic only; from northern Florida to Iceland, Ireland, and Norway, reaching to about 68°.5 N. lat. on the Scandinavian coast.

Bathymetrical Range.—From 140 to 2340 meters (77 to 1300 fathoms).

Thermal Range.—From 0°.1C. to 8°.4C. and 48°.7 Fahr.

Included Species.—*Rhizocrinus lofotensis* M. Sars, and *Rhizocrinus verrilli* A. H. Clark.

Bythocrinus Döderlein.

Bythocrinus DÖDERLEIN, Wiss. Ergeb. der deutschen Tiefsee-Expedition auf dem Dampfer "Valdivia" 1898–1899, **17,** 1912, Heft 1, p. 11 (genotype *Rhizocrinus* [*Bythocrinus*] *chuni* or *Rh.* [*B.*] *braueri*, spp. nov.).

Geographical Range.—East Indies, and eastward to the northeastern coast of Africa; Atlantic coasts of northwestern Africa and southwestern Europe; Gulf of Mexico, and southward at least to Ceará, Brazil.

Bathymetrical Range.—From 158 to 1668 meters (88 to 927 fathoms).

Thermal Range.—The records are 3°.8 and 4°.6C., and 40°.5 Fahr.

Included Species.—*Bythocrinus braueri* Döderlein, *Bythocrinus brevis* (A. H. Clark), *Bythocrinus chuni* Döderlein, *Bythocrinus conifer* (A. H. Clark), *Bythocrinus intermedius* A. H. Clark, *Bythocrinus nodipes* (Döderlein), and *Bythocrinus robustus* (A. H. Clark).

Democrinus Perrier.

Democrinus PERRIER, Comptes rendus, **96,** 1883, No. 7, p. 450 (genotype *D. parfaiti*, sp. nov.).

Geological Range.—First found in a breccia containing a human skeleton at Guadeloupe, French West Indies; otherwise only known from the recent seas.

Geographical Range.—From Sulu (Jolό) to Ceram Laut and Timor; near Krakatoa; coast of Morocco; Caribbean Sea and Gulf of Mexico.

Bathymetrical Range.—From 112 to 2050 meters (62 to 1139 fathoms).

Thermal Range.—No records.

Included Species.—*Democrinus parfaiti* Perrier, *Democrinus rawsonii* (Pourtalès), *Democrinus sabae* (A. H. Clark), and *Democrinus weberi* (Döderlein).

There are in the literature many records referring to species of this genus which are undeterminable.

ABSTRACTS

Authors of scientific papers are requested to see that abstracts, preferably prepared and signed by themselves, are forwarded promptly to the editors. Each of the scientific bureaus in Washington has a representative authorized to forward such material to this journal and abstracts of official publications should be transmitted through the representative of the bureau in which they originate. The abstracts should conform in length and general style to those appearing in this issue.

SPECTROSCOPY.—*Wave-lengths of the stronger lines in the helium spectrum.* PAUL W. MERRILL. Bureau of Standards Scientific Paper No. 302 (Bull. Bur. Stds., **14**: 159–166). 1917.

Wave-lengths of twenty-one of the stronger helium lines have been carefully measured photographically by means of the Fabry and Perot type of interferometer, using several separations. Nine of the lines were compared directly with the standard cadmium line 6438.4696A, the remaining lines being referred to these. The accuracy attained is nearly 0.001A, so that the lines are now available as convenient standards for many purposes. It is well known that the separation of the effective reflecting surfaces of the interferometer usually appears to be slightly different for different colors; the possibility of eliminating this effect is noted. The Kayser and Runge spectral series formula with constants derived from three consecutive lines will not reproduce accurately even the next member of any one of the six series.

P. W. M.

MINERALOGY.—*An American occurrence of miloschite.* EDGAR T. WHERRY and GLENN V. BROWN. American Mineralogist, **1**: 63–67. 1916.

The discovery of this mineral at Ely, Nevada, is announced, and a full description of its properties and composition given. It is shown to be a chromium-bearing kaolinite, and is considered a mineral species.

E. T. W.

PALEOBOTANY.—*Two new fossil plants from the Triassic of Pennsylvania.* EDGAR T. WHERRY. Proc. U. S. Nat. Mus., **51**: 327–329. 1916.

Descriptions of a new conifer, *Palissya longifolia*, and of a plant of unknown relationships, named *Brunswickia dubia*, from the Brunswick formation, in which it occurs. E. T. W.

PROCEEDINGS OF THE ACADEMY AND AFFILIATED SOCIETIES

THE BIOLOGICAL SOCIETY OF WASHINGTON

The 571st regular meeting of the Biological Society of Washington was held at the Cosmos Club, May 5, 1917; called to order by President HAY; 25 persons in attendance.

On recommendation of the Council, DONALD R. DICKEY, Pasadena, California, and J. EUGENE LAW, Hollywood, California, were elected to membership.

Under the heading Brief Notes President HAY exhibited a lantern slide of some very immature flying squirrels.

The regular program consisted of two communications:

F. V. COVILLE: *The influence of cold in stimulating the growth of plants.* Mr. Coville stated that the spring and summer growing period of plants in regions having cold winters is followed by a period of dormancy which persists if the plants are artificially maintained at a relatively high temperature. A period of exposure to cold is needed to activate the plants for another period of growth. The mechanism of activation appears to be the liberation of enzymes acting on the stored starches, converting them to sugars; it is perhaps a change in the permeability of the cell membrane. It is normally brought about by cold, but mechanical injury or a period of drying may bring about the necessary changes. The operation is not controlled by the roots but acts independently in any exposed parts of the plants, so that if of two branches of a plant one is kept continually warmed and the other subjected to the usual winter chilling the former will not develop on the advent of summer temperatures, while the latter develops normally. Mr. Coville's paper was illustrated by numerous lantern slides. His communication was discussed by Messrs. E. A. GOLDMAN, WM. PALMER, L. O. HOWARD, W. P. HAY, and A. A. DOOLITTLE.

W. P. HAY: *The rate of growth in certain lower vertebrates.* Professor Hay gave some of his personal observations of the rate of growth of the loggerhead turtle, and observations, based on the published records of others, on the rate of growth of certain snakes and of an alligator. His communication was illustrated by charts and diagrams.

M. W. LYON, JR., *Recording Secretary.*

THE ANTHROPOLOGICAL SOCIETY OF WASHINGTON

The 512th meeting of the society was a special meeting held on May 1 in the Natural History building of the National Museum to hear the address of the retiring president, Dr. JOHN R. SWANTON, whose subject was *Some Anthropological Misconceptions.*

Dr. Swanton began by calling attention to the cyclic nature of cultural movements and stated that like other beliefs the doctrine of evolution which so dominates the thought of our time is subject to the same law, and bound to have its rise, decline, and disappearance as an object of peculiar interest, and further, that the truth embodied in it will in time become so axiomatic that no particular attention will be paid to it and the chaff will disappear.

Unfortunately when pioneer anthropologists began to apply evolutionary ideas to their science, then in its infancy, they fell into a serious error. They assumed, with some justice indeed, that the existing peoples of the world presented features, some more and some less primitive, features which might be arranged into series showing the stages which mankind as a whole had passed through. But in selecting the "most primitive" features they worked on the false assumption that that which was most foreign to the ideas of the society in which they lived, in the cultural center of western Europe, as the most primitive. This resulted in a vast crop of pseudo-scientific evolutionary theories, each based on its author's own peculiar understanding of what was more and what less primitive. An assistant source of error was an over earnest attempt to find survivals analogous to the "vestigial characters" of biology in all kinds of cultural features, many of which were not vestigial at all. The speaker referred to several evolutionary theories of this kind, treating at some length those regarding the evolution of totemism from a matrilineal clan system, the evolution of marriage from a primitive promiscuity, and several theories concerning the origin of religion, such as those of Spencer, Tylor, Frazer, and Lang.

Secondly, the author took exception to the extreme uniformitarian attitude taken by certain anthropologists. He called attention to the fact that absolute uniformitarianism is impossible since even the inorganic world is based on discrete molecules, atoms, electrons, etc., while the organic world is based on independent organisms. In the same way when we turn to the culture history of mankind we find that ideas, although progressive, do not roll into consciousness with the even motion of a wheel, but come at certain definite times and places.

Along with this extreme uniformitarianism he believed too much stress had been placed on the unconscious or subconscious side of evolution in human institutions. Important as the latter undoubtedly is and much as it is neglected by the man of average intelligence, it acts less toward the development of new institutions than toward the preservation of institutions already in existence, and is accompanied by degeneration, or at most imitation, rather than by absolute origination.

In this connection Dr. SWANTON took occasion to criticise a certain type of student who, because he observes the powerful effect of subconscious imitation, assumes that there is an extra-mental current which settles all problems, and looks cynically upon conscious efforts to bring about change. When examined closely this seemingly unconscious current would be found to be a resultant of forces, each of which was the decision of some individual or some group of individuals at a definite time and place. He believed that if any of these decisions had been different the stream itself, the course of history, would in some measure have been different.

In the discussion which followed the address Dr. LEO J. FRACHTENBERG agreed in main with the assertions made by the speaker. He called attention to the fact that the error of particularization is well exemplified in Westermarck's "Origin of Primitive Ethics." Dr. Frachtenberg expressed the belief that the principles of unconscious evolution should not be underestimated. Dr. TRUMAN MICHELSON added as another misconception the supposition that the languages of primitive peoples indicate a low mentality, stating that it is possible to express complicated ideas by means of these languages but that the ordinary life of the people does not require such expression. Another misconception, according to Dr. Michelson, is the arranging of languages in a scale of superiority with inflectional languages as the highest point of development.

In closing the discussion Dr. Swanton stated that although it is true that the unconscious element plays a very important part in the evolution of culture, its functions are conservative or, at most, imitative, with a tendency toward degeneration, whereas the conscious element is that which creates, that which produces positive advances.

FRANCES DENSMORE, *Secretary*.

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